INDIVIDUAL FORECASTING ABILITY

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Working Paper #1903-87

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May 1987

Preliminary and incomplete. Comments welcomed.

This paper was prepared for presentation at the Seventh International Symposium on Forecasting, Boston Massachusetts, May 1987. The author gratefully acknowledges participants in the symposium for comments, without implicating them in any way for remaining errors or omissions.

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1. Introduction

Are some analysts "superstars"? Financial press coverage of analysts' performances suggests that there are superior financial analysts. One example of this coverage is the Institutional Investor "All American Research Team," based on surveys of money managers, who nominate and rank analysts. In this paper, I explore the question of individual superiority by examining one aspect of analysts' services: earnings forecasting.[1] Forecasts of future earnings are of interest to investors, as inputs to investment decisions, and also to academics, as data on earnings expectations.

Academic use of analysts' earnings forecasts as proxies for expected earnings in capital markets research is now widespread.[2] Both availability of large databases of forecasts[3] and demonstrations of analysts' accuracy at prediction[4] have aided this development. The results of this paper are useful to academics interested in using analyst forecasts as earnings expectations. If individual analysts' forecasting ability differs in a systematic way that remains reasonably stable over time, this information can be used to construct better aggregate forecasts, by forming weighted combinations. I find no

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[1]The Institutional Investor list is based on several criteria, including earnings forecasting, picking stocks, and the quality of written reports. By focusing on earnings forecasting, I do not mean to suggest that it is the only or the most important of analysts' activities.

[2]Some examples are: Patell and Wolfson (1984), Ricks and Hughes (1985), Bamber (1986), Hoskin, Hughes and Ricks (1986) and Pound (1987).

[3]Examples of such databases are: the I/B/E/S Summary data, the Standard and Poors' Earnings Forecaster, and Zacks Investment Advisory Services' database.

[4]Brown and Rozeff (1978), Collins and Hopwood (1980), Fried and Givoly (1982), O'Brien (1987).

evidence that this effort is likely to be fruitful. Rather, my results suggest that individuals are relatively homogeneous in terms of their forecast accuracy.

The criterion I apply to evaluate forecasting ability is average accuracy on the portfolio of predictions made by an individual, through time. It is reasonable to presume that some firms' earnings are harder to forecast than others'. Since analysts may, to some extent, choose to predict earnings for some firms and not others, this endogenous selection is important to the specification of statistical tests. The results reported here remove the effects differential predictability across industries in comparing analysts.

Evaluation of average portfolio accuracy <u>through time</u> is important in earnings forecast data. Each year, there is a relatively large component of unanticipated information which is common across firms, probably because of unanticipated economy-wide or industry-wide events. This common information is evident in the large, statistically significant year-by-year differences in (cross-sectional) average forecast errors. This general feature of earnings forecast data makes it difficult to distinguish between a "lucky guess" on a macro event and true forecasting ability at the micro level, when only short time-series are available.[5] Current databases are rich in cross-sectional observations, but the number of years over which forecasts are available is still relatively small. This issue, and my treatment of it, is discussed further in section 3.

The paper is organized as follows. Because the database used is not widely available, section 2 describes the data in some detail. Section 3 concerns

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[5]Forecasts of macroeconomic variables are of interest in their own right. They have been studied, for example, by Zarnowitz (1984), and others. However, my focus is on forecasting ability at the portfolio or individual firm level, which is ostensibly a component of analysts' services to their clients.

differences in portfolios of firms forecast by different individuals, and the implications for sample selection. In section 4 I describe the statistical model used to examine the relative accuracy of individual analysts' forecasts, and present the evidence from this sample. Section 5 is a summary with some concluding remarks.

2. The Analyst Forecast Data

The data are individual analyst forecasts made between 1975 and 1981, compiled by Lynch, Jones and Ryan, a New York brokerage house. Lynch, Jones, and Ryan use the individual forecasts to produce the I/B/E/S Summary database, which is sold to clients, who are primarily institutional investors. The analysts included in the database are employed by a variety of financial service institutions, such as brokerage houses and investment banks. Analysts and their employers are denoted by code numbers in the database, and I do not have access to information on their identities.

The database is updated once per month with new forecasts. The monthly lists used in this study span July 1975 through September 1982. Data items include the identity of the firm whose earnings per share (EPS) are forecast, the identities (code numbers) of the individual making the forecast and his employer, and the date the forecast was made. Although most financial service institutions (hereafter, FSIs) employ many analysts, at most one analyst's forecast is available from each FSI for a given firm's earnings at a given time. Forecasts are collected by Lynch, Jones and Ryan for the current fiscal year, and for the following year when available. I use only forecasts for the current fiscal year.

The number of firms whose earnings are forecast in the I/B/E/S database has grown considerably over time, from approximately 1000 in 1975 to more than 2500 by 1982. My initial subset of the database comprises the 508 firms with December yearends which have at least one forecast available in each year, 1975 through 1981. This requirement will impart a "survivorship" bias to the sample, since firms that started up and firms that ceased to exist between 1976 and 1981 are excluded. Also, since I/B/E/S has grown through time, the firms with forecasts available in 1975 may have been those most closely-followed by financial analysts, and therefore may be larger and/or more stable. To the extent that this was true of the database in 1975, it will be carried over into my sample.

A firm is eliminated from the sample if annual earnings are not available on COMPUSTAT in 1982. This requirement reduces the sample to 497 firms, and probably also increases the bias toward larger and/or more stable firms.

The identification of forecasts with individual analysts is a valuable feature of this database. This feature allows me to examine forecasting ability at a more micro level than has been done in previous studies. However, it is worth noting two important features of the data, and my attempts to address them in designing the empirical study. The first feature is the non-uniqueness of analyst identifier codes. The second feature is the "publication lag" in the data. These are decribed in turn below.

To be most useful for this type of study, the identifying codes for individual analysts should uniquely identify a person for the entire time period he produces forecasts, and regardless of the FSI(s) employing him. While this is generally true of the analyst identifiers in the database, I found instances of non-unique codes; that is, the same code was given to more

than one individual in the database at one point in time. However, the combination of the FSI and analyst codes together gave unique identification.

To avoid attributing forecasts made by two different people to a single person, I use the combination of FSI and analyst codes to identify individuals in this study. The drawback to this approach is that it does not allow individuals who change jobs, but remain in the database employed by a different FSI, to be tracked through time. Therefore, analysts will appear in my sample to predict for smaller numbers of years, on average, than is the case in the Real World. Because I/B/E/S is a secondary source, and I do not have access to the identities of FSIs or individuals, this limitation seems unavoidable.

The data are constructed from self-reports of forecasts by analysts to Lynch, Jones & Ryan, along with regular telephone surveys of analysts who fail to report. Analysts assign forecast dates to the forecasts, and these forecast dates are reported in the database. There is a "publication lag" between a forecast's date and its first appearance in the I/B/E/S database, which arises for the following reasons. First, because the data from which my sample are drawn are based on monthly updates of forecasts, and it is implausible that all analysts issue forecasts immediately prior to the updates, a publication lag of less than one month, on average, is inevitable. Second, an additional lag is created by (possibly unavoidable) imperfections in the self-reporting and survey process. The result is that some updates made within a given month do not appear in I/B/E/S until a later month. On average, in the 508-firm sample, over the seven years from 1975 through 1981, the publication lag is 34 trading days, or about one-and-a-half calendar months, with a standard deviation of 44.5 trading days.

In selecting the sample for this study,[6] I eliminate publication lags, by

[6]The sample selection is described in section 3.

ignoring the date of the forecast's first appearance in I/B/E/S, and using the analyst's forecast date. After eliminating publication lags, there is still considerable variation in the ages of forecasts[7] in the sample. These differences exist primarily because analysts differ in how frequently they update their forecasts. The more frequently an analyst updates forecasts, the smaller will be the average age of his forecasts included in the sample. However, unlike the publication lag, which may or may not be under the control of the analyst, and which is unlikely to be related to forecasting ability, the frequency with which the analyst updates forecasts is clearly under his control, and is more likely to be related to forecasting ability. Therefore, variation in the ages of forecasts included in the sample reflects a potentially important feature of the population from which the sample is drawn. I return to this issue in the following section.

3. Sample Selection

The sample is selected at a fixed horizon of 120 trading days, or slightly less than six calendar months, prior to the announcement of annual earnings. That is, for each firm and year, the announcement date of annual earnings is obtained from COMPUSTAT or from the Wall Street Journal. Next, the horizon date, 120 tradings days prior to the announcement, is determined using the CRSP trading day calendar. From the point of view of the horizon date, the most recent forecast from each analyst forecasting EPS for that firm and year is selected for the sample.

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[7]A forecast's age is defined as the length of time between the analyst's forecast date and the (exogenously chosen) date at which the sample is constructed. That is: from the point of view of the sample selection date, how old is a given forecast? See O'Brien (1987) for an analysis of forecast ages in a different sample from the same database.

The motivation for selecting a fixed horizon is to make the date at which the sample is constructed independent of analysts' decisions, and to make the horizons roughly comparable across firms and years. This sample of forecasts is intended to represent the population available at a fixed, exogenously determined, point in time.

The analysts' forecast data form a panel, or time-series of cross-sections. Because analysts can, to some extent, choose the firms they forecast and because of limitations on individuals' time and energy, forecast data are extremely unbalanced. That is, there is not a forecast from each analyst for each firm in each year.[8] The fact that EPS forecast data are inherently unbalanced has important implications for the sample design and statistical inferences.

A common sample design involves choosing a set of firms and a set of analysts for which complete data (in this case, a forecast) can be obtained for each time period. This sample design results in balanced data. An advantage to analyzing balanced data, in evaluating predictive ability of individual forecasters, is that all forecasters are evaluated on the same benchmark, since by sample construction each forecaster predicts the same firms' earnings in the same years.

On the other hand, this sample selection procedure can obscure important variation in the sample. For example, in the database from which this sample is drawn, to obtain balanced data on a five-year time-series, it is necessary to limit the portfolio to at most fifteen firms at one time. More importantly, comparisons are limited to at most six analysts at one time. Clearly, this

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[8]This feature is not unique to I/B/E/S. For example, a perusal of the Standard and Poors' Earnings Forecaster will verify that, even at the level of FSIs, the data are unbalanced.

limits the researcher's ability to generalize results to the larger population of analysts and firms.

The tradeoff, in selecting a sample of analysts' forecasts, is between preserving enough data to make comparisons meaningful and maintaining enough variation in the sample to allow generalization. At one extreme, if each analyst predicts EPS for only one firm in one year, then comparisons across analysts are not statistically meaningful. At the other extreme, if the sample is selected to achieve fully balanced data as described in the previous paragraph, statistical comparisons can be made, but generalization of the inferences to other analysts and firms would likely be unreliable.

An alternate way to treat unbalanced data is to adjust for the effects of the lack of balance in the statistical design. This is the approach I use. The statistical techniques are described in section 4. Sample selection criteria (4) and (5), described below and in Table 1, are attempts to address the tradeoff discussed above: retaining as much variation as possible, while allowing statistical comparison.

The first two sets of criteria listed in Table 1 are those described in the previous section, resulting in 497 firms. Criterion (3) describes the set of forecasts available in the I/B/E/S database at the 120-trading-day horizon for the sample firms. There are 38611 forecasts of 457 firms' EPS from 3041 analysts at this stage. The effects other sample selection criteria on the number of forecasts, firms and analysts in the sample are displayed in Table 1 and discussed below.

Criterion (4), that analysts in the sample have at least 30 forecasts, for any firms in any years, eliminates nearly 90% of all analysts in the database. However, note that the remaining approximately 10% of analysts produce nearly half of the forecasts. Criterion (4) is clearly affected by the conservative

method of uniquely identifying analysts, discussed in the previous section. The method limits the possibility of attributing forecasts made by two different individuals to a single analyst, but results in understatement of the number of forecasts available from individuals who change jobs. The reason for criterion (4) is to ensure sufficient information is available to generate statistically viable comparisons.

The fifth criterion, that each analyst in the sample have forecasts in at least four of the seven years for which data exist, again addresses the issue of statistical validity. As was discussed previously, the shorter the time-series on a given analyst, the harder it is to distinguish true forecasting ability, because of common information across firms within years.

Criterion (6) attempts to address a possible flaw in the database. As mentioned above, at any point in time, there is variation in the ages of forecasts available. This variation may occur in the population for several reasons. First, if some analysts update their forecasts only in response to new information, old forecasts may remain because (1) for some firms, there has been no important unanticipated information, or (2) the analyst is a superior forecaster and foresaw events which others did not, hence no updating was necessary for that individual. Second, analysts who are less diligent or less interested than their peers may update less frequently. Both these sources of variation in age of forecasts are features of the population that may be related to forecasting ability, and therefore are desirable to retain in the sample.

A third possible source of variation in forecast ages is attributable to the data collection process, and therefore is not desirable to retain in the sample. Forecasts, once submitted, are retained in the database until they are (1) updated by the analyst, (2) replaced by another forecast from a different

analyst at the same FSI, (3) withdrawn by the analyst,[9] or (4) made obsolete by the announcement of actual EPS for that firm and year. If, however, the analyst or FSI stops submitting forecasts to I/B/E/S, the forecast might inadvertently remain in the database.

The effects on the sample of eliminating forecasts on the basis of forecast age, given all the other sample selection criteria, are displayed in Table 2. The goal, in applying an age filter, is to eliminate the very oldest forecasts, which are most likely to be artifacts of the data collection process, without eliminating the desirable variation in ages in the sample. An interesting feature to note in Table 2 is that the variation in ages of forecasts in the sample is primarily attributable to differences across analysts, not across firms. This is evident from the fact that reductions in the number of forecasts from successively stronger age filters are roughly proportional to reductions in the number of analysts, and not to reductions in the number of firms.

Criterion (6) in Table 1 is a filter on forecast age. Forecasts which are more than 200 trading days (about 9 calendar months) old on the horizon date are eliminated. Twenty-two analysts are eliminated from the sample by this criterion.

In the sample that results from these criteria, there are 10,586 forecasts from 191 analysts for 428 firms. The major industries, defined by 2-digit SIC codes from COMPUSTAT, represented in the sample are displayed in Table 3. The data are not concentrated by industry. Not surprisingly, some industries appear to be more closely-followed by analysts than others. For example, the

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[9]This can occur, for example, with an important new event, such as a tender offer or a strike, which may alter estimates substantially. The analyst will temporarily withdraw his forecast to study the implications of the new event, and issue a new one later.

Petroleum refining industry (SIC 29) accounts for only 4.7% of the firms in the sample, but represents 10.9% of the forecasts. On the other hand, Electrical and electronic (SIC 36) with 5.4% of the firms, has only 2.9% of the sample forecasts.

Eight of the 10,586 forecasts, identified as probable data errors, are eliminated from the sample. These eight are distinguished from other "outliers," or large forecast errors, by comparison with other forecasts for the same firm and year, and with other forecasts by the same analyst for other firms and other years. For each of these eight observations, the forecast error was: more that $10.00 per share in absolute value; more than 150% of the prior year's actual EPS; different from other analysts' forecast errors for the same firm and year by an order of magnitude or more; and different from the analyst's average accuracy in other forecasts by an order of magnitude or more. Such errors could occur, for example, if a forecast of $2.40 per share were inadvertently entered into the database as $24.00 per share.

In the next section, the statistical design to compare portfolio performance across analysts through time in these data is described.

4. Statistical Tests

All statistical tests in this paper are based on forecast errors, computed as the difference between primary EPS for a given firm and year as reported in COMPUSTAT, A_{jt}, and the analyst's forecast of those EPS, F_{ijt}:

$$e_{ijt} = A_{jt} - F_{ijt} \quad . \qquad (1)$$

In (1), the subscript i indexes analysts, j indexes firms and t indexes years. When a forecast is for fully-diluted EPS, instead of primary, the forecast

number is converted from fully-diluted to primary using the ratio of fully-diluted to primary EPS for that firm and year from COMPUSTAT. When a stock split or stock dividend is announced between the forecast date and the announcement date of EPS, the forecast is adjusted for the capitalization change.

Forecast errors constructed as in (1) are expected to have large components of common information cross-sectionally within years, as a result of unanticipated macroecomic events which affect many firms in the same way. This is especially true over long time horizons, like the one used here of approximately 6 months.[10] Examples of macroeconomic events which can affect many firms in similar ways are unanticipated changes in interest rates, inflation, or oil supply conditions.

The year-specific components of forecast errors in this sample are constructed using the following model:

$$e_{ijt} = \mu_t + \varepsilon_{ijt} \quad . \tag{2}$$

The μ_t, or annual average forecast errors, in equation (2) are estimated using OLS on a set of dummy variables for the seven years in the sample. The results of this estimation are reported in Table 4. It is evident from Table 4 that year-to-year differences in average forecast errors exist, and are statistically significant. The explicit statistical test for homogeneity of average forecast errors across years is the reported F-statistic of 35.99, which rejects the hypothesis of homogeneity. This test assumes that deviations of forecast errors from their annual averages, the ε_{ijt}, are independent. If

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[10]See O'Brien (1987) for evidence that this common information component increases as the forecasting horizon lengthens.

adjusted for the possibility of heterogeneity among analysts and among 2-digit SIC industries,[11] the test statistic becomes 33.44, which also rejects the null.

Statistical tests to compare analysts' forecasting abilities are conducted using the ε_{ijt}, the deviations of forecast errors from the annual averages. Essentially, this purges forecast errors of aggregate unanticipated year-specific information. This adjustment is important for two reasons, one of which is related to the unbalanced nature of the data. First, given the evidence in Table 4 that years are not homogeneous, if the year-specific information is ignored, comparisons will depend upon the years involved. For example, imagine one analyst with forecasts only in 1975, 1978, 1979, and 1980, the four years with the smallest average forecast errors. Imagine a second analyst with forecasts only in 1976, 1977, 1979, and 1981, the four years with the largest average forecast errors. Assume each has (for simplicity) one forecast in each year, and each is of average forecasting ability, as defined by the numbers in Table 4. If forecast errors were assumed homogeneous across years, then the first analyst's average squared forecast error would be .0072, while the second analyst's would be .0645.[12] The first analyst's apparent greater accuracy is entirely attributable to the years involved, since each is precisely average in forecasting ability. Clearly, this feature exists only because we seek to make inferences from unbalanced data. If both analysts had

[11]The test was also done allowing for heterogeneity at the firm, rather than industry, level. The resulting F statistic was between the two values reported here.

[12]Root-mean-squared errors may be easier to interpret here, since they are expressed in dollars per share, as are EPS numbers. The root-mean-square errors for this example are $0.08 and $0.25 per share for the first and second analysts, respectively.

forecasts of average accuracy in each of the seven years they would be indistinguishable.

The second effect of ignoring annual average forecast errors in statistical comparisons is that standard errors will be mis-stated, because of cross-sectional correlation within years. This is evident from inspection of equation (2). An annual average μ_t is a component of forecast errors e_{ijt}, common to all analysts and firms in year t. The ε_{ijt} are purged of this common information.

The search for superior ability to predict EPS is based on ε_{ijt}, the forecast errors purged of year-specific common unanticipated information, or deviations about the annual average. The criterion I use to examine accuracy is average squared deviation, or the average of $(\varepsilon_{ijt})^2$, estimated in a fixed effects model:[13]

$$(\varepsilon_{ijt})^2 = \mu_{1i} + \mu_{2k} + \mu_{2t} + \zeta_{ijt} \tag{3}$$

In equation (3), i, j and t are defined as before, as indexes of analysts, firms and years respectively. The index k denotes the 2-digit level industry to which firm j belongs. The estimated effects, μ_1, μ_2 and μ_3, are interpreted as follows. μ_{1i} is the average squared error accuracy of analyst i, conditional on the industries and years in the sample that are associated with analyst i. That is, μ_{1i} measures the accuracy of analyst i on the portfolio for which he forecasts earnings, through time. μ_{2k} is the average squared error accuracy for industry k, conditional on the analysts and years in the

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[13]Searle (1971) contains a thorough discussion fixed effects models and their applications for unbalanced data. See Mundlak (1978) for a general exposition in economic contexts.

sample.[14] μ_{3t} is the average squared error accuracy in year t, conditional on the analysts and firms in the sample.

The reason for including firm and year effects in equation (3) is to control for inherent differences in accuracy, reflecting differences in predictability, among industries and years. Differences in predictability among industries arise if there is more uncertainty about earnings in some industries than in others, and the differences persist over the sample time period. Differences in predictability across years can arise if there is more aggregate uncertainty in some years than in others. Recall that the dependent variable, ε_{ijt}, is purged of annual average forecast errors. The μ_3 measure year-by-year average dispersion around the annual means.

The form of equation (3) is convenient for statistical testing. Tests for homogeneity in accuracy are linear tests on groups of parameters. The test for homogeneity in forecast accuracy among analysts, for example, is an F test that the μ_1 all are equal. The results of homogeneity tests in equation (3) are reported in Table 5. The tests reject homogeneity among industries and across years, but fail to reject homogeneity among analysts. That is, there is evidence in this sample that forecast accuracy differs by year and by industry, but there is no evidence that forecast accuracy differs across the 191 analysts in the sample.

5. Summary and Conclusions

[14]The model was estimated using firm effects in place of industry effects. The results are very similar to those reported here. Industry effects are slightly more significant than firm effects.

In this paper, I have considered the question of individual forecasting ability by examining forecasts of earnings per share made by 191 analysts in the period 1975 through 1981. There is no evidence in this sample that analysts differ in forecast accuracy to a statistically distinguishable extent. Forecast accuracy is defined here as mean squared error on the portfolio of firms for which the analyst produces forecasts, through time, controlling for differences in predictability among industries and across years.

There is evidence in the sample that average annual forecast errors vary significantly from year to year. This is expected, especially over longer forecasting horizons, due to unanticipated macroeconomic events which affect many firms in a similar manner, and which differ from year to year. Analysts' forecast accuracies are therefore measured in terms of deviations around the annual average of all forecast errors.

Some caveats are worth noting about the results. While the number of analysts included in the sample is moderately large, the necessity of sufficient data for reliable estimates excluded the vast majority of the analysts in the database from the sample. To avoid attributing forecasts made by two different individuals to a single one, a conservative approach to identifying analysts is employed. This approach almost surely results in the exclusion of analysts who change jobs frequently from the sample. If analysts who change jobs frequently, but who remain in the database employed by different financial service institutions, do so because they are superior analysts who are bid away by competing firms, then the identification and sample selection processes may exclude these superior analysts.

I have examined only one component of analysts' services to their clients, ability to forecast earnings. Forecasting ability is worthy of study, because earnings forecasts are useful to academics as well as to investors. From a

practical standpoint, earnings forecasts possess advantages of relative objectivity and quantifiability, and of data availability. It may be the case, however, that analysts are valued more for their other services to clients, such as buy/sell recommendations, than for their earnings forecasting ability. If it is the case that analysts are not compensated for producing highly accurate forecasts, then it is not surprising that analysts are indistinguishable in terms of forecast accuracy.

TABLE 1

The Effects of Sample Selection Criteria

Selection criteria:	# Firms	# Forecasts	# Analysts
(1) December 31 yearend, with at least 1 forecast in I/B/E/S in each year, 1975-1981	508	n.a.	n.a.
(2) Annual EPS available on 1982 Compustat	497	n.a.	n.a.
(3) Analyst forecast(s) available in I/B/E/S 120 trading days before yearend	457	38611	3041
(4) Analysts with at least 30 forecasts	445	16970	323
(5) Analysts in (4) with forecasts in at least 4 years	433	11787	208
(6) Analysts with at least 30 forecasts in at least 4 years, no forecast more than 200 trading days old	428	10586	191

TABLE 2

Given other selection criteria*, how does selection on forecast age alter the sample?

	Forecasts		Analysts		Firms	
Criterion	#	% sample reduced	#	% sample reduced	#	% sample reduced
No age selection	11787	0.0%	208	0.0%	433	0.0%
Age ≤ 200 days**	10586	10.2	191	8.2	428	1.2
Age ≤ 100 days	7343	37.7	142	31.7	411	5.1
Age ≤ 50 days	2786	76.4	56	73.1	336	22.4
Age ≤ 25 days	677	94.3	11	94.7	258	40.4
Forecasts made since 2nd quarter EPS announcement	268	97.7	4	98.1	162	62.6

* Other selection criteria are: analysts must have at least 30 forecasts in at least 4 years in the I/B/E/S database, for firms with December 31 yearends with earnings data on COMPUSTAT.

** Forecast ages are measured in trading days, as defined on the CRSP trading day calendar.

TABLE 3

Industry Representation in Sample

2-digit SIC code	Industry name	Forecasts #	Forecasts %	Firms #	Firms %
26	Paper	650	6.1%	17	4.0%
28	Chemicals	1533	14.5	41	9.6
29	Petroleum refining	1150	10.9	20	4.7
33	Primary metals	350	3.3	10	2.3
35	Machinery	507	4.8	29	6.8
36	Electrical & electronic	306	2.9	23	5.4
49	Elec., gas & sanitary svcs.	1892	17.9	63	14.7
60	Banking	1073	10.1	33	7.7
63	Insurance	314	3.0	9	2.1
		7775	73.4%	245	57.2%
other	40 2-digit industries	2811	26.6	183	42.8
total		10586	100.0%	428	100.0%

TABLE 4

Average Annual Forecast Errors*

Year	# of Observations	Mean forecast error	Standard error of mean
1975	694	$ 0.00	$ 0.05
1976	1118	-0.21	0.04
1977	1400	-0.25	0.03
1978	2048	0.08	0.03
1979	1931	0.12	0.03
1980	1702	-0.09	0.03
1981	1685	-0.37	0.03

$F(6, 10571) = 35.99$**

* The average forecast errors, measured in dollars per share, are computed via least squares by pooling across firms and analysts within years.

** The F-statistic tests rejects the hypothesis of homogeneity in annual averages at conventional significance levels.

TABLE 5

Tests for Homogeneity in Forecast Accuracy*

Source of Variation			
Year	F(6, 10333)	=	5.64**
Industry	F(48, 10333)	=	5.97**
Analyst	F(190, 10333)	=	0.94***

* The homogeneity tests are based on a fixed effects model, with forecast accuracy defined as average squared deviations from the annual mean forecast error.

** Homogeneity rejected at conventional significance levels.

*** Homogeneity not rejected.

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